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A POSSIBLE EXPERIMENTAL DESIGN FOR TESTING THE EFFECT OF ARTIFICIAL CLOUD SEEDING

Submitted by G. P. Wadsworth, Project Director

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13. ABSTRACT

Because of the clash of opinion on the effectiveness of artificial cloud seeding as a means of increasing precipitation, an objective method of appraisal is imperative. The problem is herein shown to be statistical, and the general philosophy of valid statistical inference is elucidated. In the light of this background, the peculiarities of meteorological data are pointed out, and the pitfalls of ordinary procedures are discussed. An experimental design capable of resolving the present controversy is presented in the Appendix, and evidence of its soundness and power is cited in the text.

ABSTRACT

Because of the clash of opinion on the effectiveness of artificial cloud seeding as a means of increasing precipitation, an objective method of appraisal is imperative. The problem is herein shown to be statistical, and the general philosophy of valid statistical inference is elucidated. In the light of this background, the peculiarities of meteorological data are pointed out, and the pitfalls of ordinary procedures are discussed. An experimental design capable of resolving the present controversy is presented in the Appendix, and evidence of its soundness and power is cited in the text.

PERSONNEL

During the period covered by this report the project was staffed by a full-time meteorologist and an assistant meteorologist, five full-time computers, and one half-time statistician. Also a research assistant was employed half-time to investigate the literature and conduct numerous pilot studies. Continual contact was maintained with two scientists from the Geophysical Research Directorate, Messrs. Charles E. Anderson and Benjamin Davidson. Weekly conferences were held with staff members. In addition, a conference was held in Washington with Weather Bureau personnel and with Colonel Benjamin G. Holzman; a personal interview was had with Dr. Wallace E. Howell; and a conference was held with Captain Philip D. Thompson and Captain William L. Jones.

A POSSIBLE EXPERIMENTAL DESIGN
FOR TESTING THE EFFECT OF ARTIFICIAL CLOUD SEEDING

Because of the inherent variability of rainfall, it is very difficult to determine whether or not the artificial seeding of clouds has produced precipitation over and above that which would be expected naturally. Current arguments pro and con attest to this difficulty. Much disagreement can be avoided if the rules of experimental procedure and interpretation are laid down in advance, for controversies are more difficult to resolve if the data are analyzed after the fact. For this reason, we have endeavored to develop an experimental design of such nature that a given outcome can be appraised objectively on the basis of the probability of obtaining such a result by chance, if seeding were assumed ineffective.

For purposes of this report, we consider it unnecessary to present detailed masses of meteorological information, since much of this is common knowledge to all who have analyzed weather records. Drawing upon well established facts, we propose to discuss the logical and statistical issues involved in the design of experiments capable of determining, with reasonable assurance, whether or not seeding causes a significant increase in rainfall. Besides giving explicit directions for applying the technique which we have devised, we cite evidence that its underlying assumptions are sound and show that it is sensitive enough to detect even a relatively small increase over natural precipitation.

If it were possible to predict with perfect accuracy how much precipitation would occur in the absence of seeding, it would be a simple matter to determine the effect of seeding. Again, even without an accurate forecast of rainfall aside from artificial modifications, it might still be possible to evaluate the contribution due to seeding, if the ensuing physical processes were precisely known. Unfortunately, neither condition is satisfied at present, and in default of both, the problem of testing the effect of seeding is statistical. The natural pattern of rainfall is extremely variable from one storm to another, and even monthly averages are highly divergent from year to year; nevertheless, under natural conditions, there exists a certain probability distribution of rainfall. If seeding were effective, the observed rainfall would show a greater tendency toward larger values than under natural conditions. The technique of

determining whether or not a real change has taken place comes under the general heading of the testing of statistical hypotheses. The province of experimental design is to provide settings wherein valid tests can be made.

The statistical theory of experimental design boils down to the construction of a test variate satisfying two conditions:

1. The test variate measures the relevant effect of the treatment in question.

2. Its probability distribution is known under the null hypothesis that the experimental treatment does not affect the test variate.

In ordinary analyses of observations drawn from a specified population, the task of constructing an appropriate test variate is relatively simple, since one can usually hypothesize the independence of individual observations, or at worst, a form of dependence which presents no analytical obstacles. The aim of the technician is then to choose from several valid tests one which has maximum sensitivity. Having performed the experiment and computed the statistic previously decided upon, one passes judgement as to the tenability of the null hypothesis. If the value of the test variate is found to lie very far outside of the common interval of variation, the null hypothesis is rejected, and one concludes that a significant effect has taken place.

Let us review the two conditions upon which a valid test depends. First, the measurement must be relevant. This requirement rules out both incidental phenomena and significant effects apart from the experimental treatment. Any action whatever must have some kind of result, but unless the result obtained is of the kind intended, it is beside the point. For example, when 13 playing cards are dealt from a standard deck without jokers, it is certain that the outcome will be a legitimate hand of bridge. Now, the probability of obtaining any preassigned hand as a random draw from the deck is about 10^{-22}, and if the hand were specified in advance, the event of drawing it would be overwhelmingly significant of skill. Unforseen as a particular entity, the same hand signifies nothing more nor less than the fact that some permutation is bound to occur. A continuous variate presents this situation in its logical extreme. Here, provided the distribution function is continuous, the probability of obtaining any preassigned value exactly is zero; therefore, one considers the

probability of obtaining a value within a stated neighborhood of a given point. Without reference to actual data, it must be decided how the test variate will be interpreted with regard to the intended operation of the treatment. This is a logical issue which can be settled a priori. For instance, if the purpose of the treatment is to increase a certain variable, then it is appropriate to interpret the experimental result in relation to the class of all values equal to or greater than the one obtained. Once an admissible standard of interpretation has been defined, and a given experimental outcome proves to be highly improbable on the basis of the null hypothesis, the rejection of the null hypothesis is then reasonable. On the other hand, a significant influence might be exerted by some dominant factor other than the treatment deliberately applied. Where it is felt that a situation of this sort might exist, great care must be exercised in the construction of the test variate, in order that the measured effect be properly attributable to the experimental treatment alone. This is one of the most vulnerable points in the design of experiments. Techniques of controlling or at least accounting for extraneous sources of variability are introduced to prevent inflation of the supposed effect of the experimental treatment. Such techniques, when correctly employed, have the further advantage of making the test more sensitive. In addition to direct methods of control, a safeguard against unsuspected or unmanageable factors is supplied by randomization. Randomization does not eliminate disturbing influences but corrects for them in a probability sense by affording an opportunity for the test variate to be decreased as often as increased by their presence. A more cogent reason for randomization, however, is that the whole structure of the statistical testing of hypotheses is built upon the concept of random sampling.

The second condition for validity is that the probability distribution of the test variate be known under the null hypothesis. To meet this requirement, it is ordinarily necessary to proceed in steps. The measurements that can be made directly are usually found to involve several sources of variation at once. The first step, therefore, is to design the experiment in such a way that the net effect of the experimental treatment can be derived from the direct measurements. As a rule, the probability distribution of the synthetic variable thus constructed will be known theoretically in its general character, but there will be some unknown parameters which will have to be specified before the distribution function can be evaluated quantitatively. Almost without exception, however, it is impossible to determine the exact value of a parameter from ob-

servational data. Therefore, the statistician proceeds to the next stage of his solution. Through a combination of ingenuity and good fortune, it frequently happens that one can find a function which incorporates the synthetic variable previously derived in such a way as to preserve the property of relevance and yet eliminate the unknown parameters, without introducing any new ones. The probability distribution of the resulting quantity will then be determinate. A function of this sort is called a parameter-free variate and is the only type suitable for the exact testing of statistical hypotheses where the parent distribution has unknown parameters. In the construction of a parameter-free variate, the statistical independence of the component parts is so nearly essential that one seldom encounters a solvable problem wherein the components are not independent. To derive the probability distribution of a function of two or more variables, their joint distribution must be utilized either explicitly or implicitly. Now, the joint distribution of statistically independent variates can be obtained immediately from their separate distributions. Otherwise, the joint distribution function would have to be known in its own right, and this would be a remarkable coincidence.

Quite understandably, many people distrust conclusions based upon statistical evidence. Their suspicion is due in part to previous experience with fallacious arguments and faulty handling of data, but it is due in large measure also to the inherent subtlety which characterizes the field of probability. Without being able to place the tag of sophistry upon any particular step of a demonstration, one frequently feels an intuitive awareness that a proof is unsound. Barring acts of deliberate misrepresentation, such as culling the data to find support for a preconceived thesis, one still can vitiate a statistical argument by failing to observe the conditions upon which validity depends. But if disturbing influences have been eliminated from the analysis, so that the test variate measures what it purports to measure; if the correct probability distribution has been employed; and if the sample has been drawn with due regard for randomization--then it is unreasonable to deny the conclusion reached. For under these conditions, the result for the data in question will be unique, and all competent statisticians will obtain the same answer.

In common with other meteorological elements, rain is distributed in a nonrandom fashion both in space and in time. True, the climatological distribution of rainfall at a given point incorporates the existing serial corre-

lation, so that the empirical probability of the occurrence of any stated range of values is faithfully represented by the corresponding climatological relative frequency. Nevertheless, the joint probability of the occurrence of a particular set of values cannot be derived from the climatological distribution of individual observations, because the data are not statistically independent, and at the present time too little is known about their stochastic relationships. Therefore, the problem of obtaining a test variate having a known probability distribution is complicated. Where individual values of a sequence are not collectively at random, striking phenomena are apt to happen. Thus, apparent periodicities are easily found in restricted portions of a time series, and well defined patterns show up in spatial distributions. Such events are definitely not in harmony with statistical independence, and, of course, a statistical test involving the tacit assumption of independence will indicate a significant divergence from the null hypothesis. With reference to precipitation, for instance, relatively large amounts of rainfall almost always occur in some locality or other during nearly every rainstorm of wide extent, and unless one pinpoints in advance the area being considered, a supposed effect can usually be found.

Besides the difficulties caused by statistical dependence, the question of proper allocation of effects is a serious one. Unless the amount of rainfall that would occur naturally can be taken into account in some equitable way, the effect of seeding cannot be appraised.

In designing experiments to study the effect of seeding upon rainfall, one is first led to the idea of setting up a control area and an area to be seeded. It would be advisable, of course, to determine from climatology the characteristics of these two areas and try to adjust them so that they are as nearly as possible equally affected by any given rainstorm. This involves the problem of how large the areas should be and also how close together they should be. It is often suggested that one area be used as a prediction for the second area, so that an estimate might be obtained as to what should occur in the second area, assuming no effect of the seeding. From the basic climatology of the areas involved, it is possible to compute linear or nonlinear regression functions connecting various groups of stations, as well as the conditional and simultaneous probabilities of the occurrence of rainfall at any two stations or areas as functions of the distance between them. This form of analysis was examined in great

detail and rejected on the ground of one serious objection which makes this type of experimentation undesirable, even when the areas to be seeded are decided each time by the toss of a coin. Although statistically it is possible to compute overall correlations or probabilities from extended records, the overall values are poor approximations to current stochastic relationships. Both the correlations and the probabilities vary widely from year to year as functions of the weather processes which are prevalent at the time considered. Consequently any experimentation utilizing this sort of technique must be continued over a good many years, in order to take into account the variations in the parameters from year to year. It must be remembered that there exist such things as weather processes which are the dominant feature in determining the regression lines for a specific period, and therefore, the amount of rainfall at various stations depends upon these characteristics. For this reason, the area being considered must itself act as a control area as well as an area of seeding, in order to carry out the experiments in a reasonable length of time.

When the Geophysical Research Directorate approached us with a specification for this report, they intimated that the work should be done so that the possibility of conducting these experiments during the latter part of the summer of 1951 was not excluded. For this reason, the actual computation and analyses were made on 37 years of data for the summer months, June, July, and August, although our experience, which has been relatively great, would indicate that without doubt the same difficulties would occur during any period of the year, and the data would perform analogously. In order to have available for analysis large areas which were relatively free of orographical and sea effects, we chose Iowa and South Dakota as locations. We examined very closely the distribution of rainfall over a square area 165 miles on a side, where the recording stations were very dense. The area was subdivided into a square grid 15 miles on a side, and the rainfall values at the intersection points of the grid were estimated by interpolation. This was done for hourly, weekly, and monthly observations. The emphasis was placed upon hourly data, since the behavior of individual storms could be studied most effectively.

Attempts were made to characterize the precipitation patterns by utilizing orthogonal polynomials—a technique which had proved fruitful in previous work with pressure maps. Unfortunately, the precipitation patterns were so complicated that a workable number of polynomials failed even to represent more than

70 percent of the variability, even when the data were smoothed in a reasonable fashion*. Furthermore, when large amounts of rainfall were added to various sections of the grid, in order to represent a hypothetical seeding effect, it was not possible to observe a characteristic change in the polynomials. Consequently, the polynomials could not be used to indicate changes in the rainfall distribution.

It would readily occur to anyone that working with smaller areas would mitigate the difficulty of characterization, and that perhaps simple tests of significance could then be devised. Granting this, it is still difficult to determine what size the area should be; for there must be room enough to seed one portion and reserve the rest for control, and yet the area must be compact enough to permit a feasible analytical representation of the observed rainfall, under natural conditions. However, it is along this direction that we finally decided upon a test which appears to be satisfactory. Standard experimental designs were critically examined, and even some nonlinear hypotheses were explored. Particular consideration was given to these designs which lend themselves to the use of mean square successive differences and other schemes for eliminating pronounced local trends in the rainfall pattern. All of these experimental designs were found to be unsound in the light of the observed behavior of rainfall, if the area were at all large.

After the examination of a great deal of data in the area chosen, the plan presented in the Appendix appeared to be the only workable one which combined a tenable hypothesis with a sufficient reduction of the meteorological variability, so that if seeding has any effect it should be observable with a reasonable number of experiments. Particular details of the experiment are open to change and can be modified in ways that seem practical. It is, of course, possible that the actual seeding cannot be performed adequately for this type of configuration and that adjustments will have to be made in the light of practicality, which will reduce the efficiency of the test.

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*In the method ultimately developed, smoothing was not employed.

The final plan, which seems most adaptable to the experiments, utilizes a square grid containing nine observation stations, located about 15 miles apart. The station at the center of the grid and the one in the middle of the edge which is directly downwind are the two stations to be seeded. The actual rainfall to be expected at these two stations, exclusive of seeding effects, can be estimated very well from the remaining seven stations. Therefore, the deviations of the actual from estimated values at these two stations provide the basis for the test variate.

In our view, this design eliminates the cause of statistical dependence. We identify the spatial correlation from point to point with an underlying continuous geometrical distribution of rainfall existing at the time of observation, and we attribute the serial correlation to the persistence of this pattern. Therefore we have hypothesized that the removal of the geometrical idealization would leave residuals which are individually and collectively at random. Lacking rainfall observations located exactly in a square array, we checked the hypothesized distribution of the test variate as best we could with data read from contour maps of actual rainfall. We wish to call explicit attention to this fact and recommend that a few trials be made with direct observations without seeding. Using 50 sets of nine values, we found that the Kolmogorov-Smirnov test for goodness of fit* supported the hypothesis at better than the 20 percent level of significance.

Many of these sets were taken from contiguous (though nonoverlapping) areas and from consecutive hours, but we advise a different sampling procedure in field practice. If a great many observations were taken over the experimental area, the shape of the geometrical surface could then be determined within any preassigned tolerance. In that event, it should not matter how dense the samples are in space or time. As a practical expedient, however, we have approximated the true surface by a properly oriented plane. This approximation should be quite satisfactory for the limited area considered, but it introduces the possibility of a small systematic error, if the samples are too close together. Therefore, randomization should be employed in actual operations.

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*Frank J. Massey, Jr.: "The Kolmogorov-Smirnov Test for Goodness of Fit", Journal of the American Statistical Association, Vol. 46, No. 253, March, 1951, p. 68-78.

With regard to the degree of approximation attainable with a plane surface, we have found that the multiple correlation is very high. In about 35 percent of the cases, the multiple correlation exceeded .95; in about 55 percent, it exceeded .90; in about 65 percent, it exceeded .85; and in about 75 percent, it exceeded .80. Accordingly, it would be feasible to restrict the formal analysis to those samples for which the plane surface is a good fit. This can be done by having a few samples to spare, and it does not violate any principles of rigor, because the selection will be made entirely without reference to the amounts of rainfall at the test points.

If seeding increases rainfall to any appreciable extent, the test variate should respond unmistakably. The kind of response available in a typical sample is exhibited in Table I. Here the value of the test variate was computed in five cases from rainfall data obtained as previously stated.

Table I

Values of Test Variate Under Three Conditions

Case	Natural Conditions	Addition of .05 in. at Test Points	Addition of .10 in. at Test Points
1	-.42	3.40	6.17
2	1.75	2.90	3.92
3	-.94	4.91	9.69
4	.45	3.33	5.42
5	-.76	7.83	13.60

Then the original rainfall at the two hypothetically seeded points was arbitrarily increased by .05 in., and the corresponding value of the test variate was recomputed; finally, the original rainfall at the two points was arbitrarily increased by .10 in., and the corresponding value of the test variate was computed again. The test variate is strongly affected in every case, although not to a uniform degree, inasmuch as the internal variability of the actual data is also involved. The probability levels associated with each value of the test variate are shown in Table II. Even the individual values obtained from the modified rainfall amounts are significant; the collective significance levels*, however, would

*The collective significance level is not the product of the separate probabilities. For exposition, see Appendix.

Table II

Associated Probability Levels

Case	Natural Conditions	Addition of .05 in. at Test Points	Addition of .10 in. at Test Points
1	.65	.014	.0018
2	.08	.022	.0087
3	.80	.004	.0003
4	.34	.014	.0028
5	.75	.001	.0001

be utterly conclusive. For natural conditions, the collective significance level is .52, which comfortably sustains the null hypothesis; for the addition of .05 in. the collective significance level is about 2.4×10^{-7}, which indicates that the null hypothesis is most definitely untenable; and for the addition of .10 in., the collective significance level is about 8.5×10^{-11}, which is so small that it would be preposterous to entertain the null hypothesis.

One might ask whether .05 in. and .10 in. represent uncommonly large hourly rainfall amounts in themselves. In the area considered, the empirical distributions of hourly rainfall during the month of July were obtained for five regular weather stations. Table III presents the relative frequencies with which the hourly rainfall, when it does rain, equals or exceeds .05 in. and .10 in. respectively, as well as the same information for .15 in. and .20 in. From the table, it is evident that an hour's accumulation of rain frequently amounts to as much as .05 in. or .10 in.

Table III

Relative Frequencies with which July Rainstorms at Five Stations in Iowa Produce Hourly Amounts of Stated Magnitudes

Station	At Least .05 in.	At Least .10 in.	At Least .15 in.	At Least .20 in.
Burlington	.51	.33	.26	.21
Coon Rapids	.44	.26	.18	.13
Des Moines	.43	.29	.20	.14
Keokuk	.49	.33	.20	.16
Washington	.56	.37	.26	.19

Because the proposed experimental areas are sufficiently small, they can be located in many places, and any number of them can be pooled to obtain a combined measure of significance, as described in the Appendix. Although this set-up is probably not the only one which might work, we hold that any valid system would have to preserve the essential attributes of this design.

APPENDIX

MATHEMATICAL ANALYSIS OF PROPOSED EXPERIMENT

The contemplated statistical test calls for a criterion variate which is approximately normally distributed. Hourly rainfall is a cross between a discrete and continuous variate, having a fairly large probability of the occurrence of exactly zero, an appreciable probability of a trace of rainfall, and in the measurable range, a skewed probability density function of rainfall as a continuous variate. To obtain a statistically manageable variate, we shall exclude from consideration values of hourly rainfall less than .05 in. and to reduce the skewness we shall work with the natural logarithm of the observed rainfall.

As a compromise between the requirements of keeping the test area small enough to be statistically homogeneous and large enough to support a subdivision into seeded and nonseeded portions, we have chosen to work with experimental plots containing nine observation points. Ideally these plots should be square in shape and subdivided into quadrants, with the observations lying at the corners of the grid squares, as shown in Figure 1. The actual orientation of the plot must be

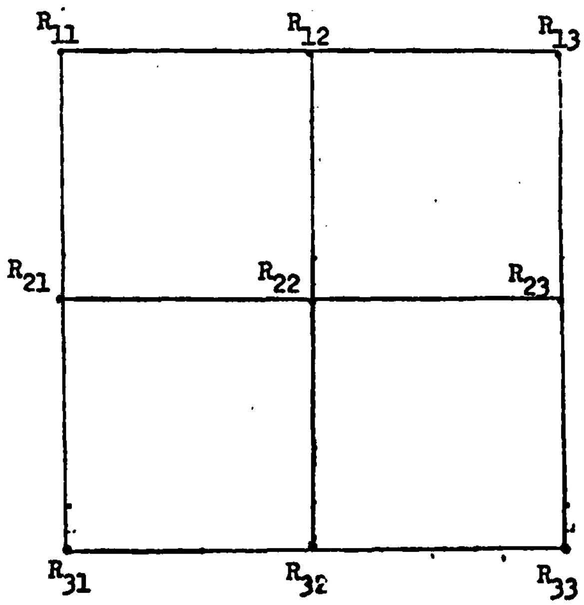

Figure 1

such that the prevailing wind direction during the hour of observation is parallel to the line segment $R_{22}R_{12}$, with R_{12} downwind from R_{22}. We shall consider first the ideal pattern and later take up the modifications necessary in case the square grid becomes operationally impracticable.

In Figure 1, the symbols R_{ij} ($i,j = 1, 2, 3$) represent the nine observations of rainfall and indicate, at the same time, the locations of the observation points. Choose a rectangular coordinate system with origin at the center of the square (R_{22}), the X-axis coinciding with the line $R_{21}R_{22}R_{23}$, and the Y-axis coinciding with the line $R_{12}R_{22}R_{32}$. Taking the side of a grid square as the unit of length, we set up the X-Y coordinate pattern shown in Figure 2.

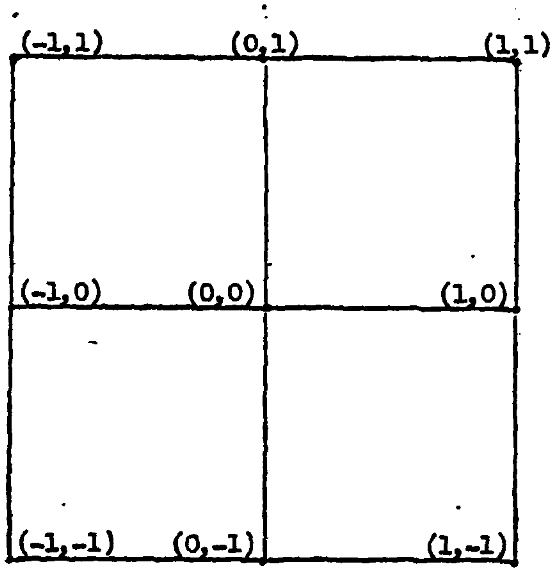

Figure 2

Within the test plot, the natural logarithm r_{ij} of the rainfall R_{ij} at the point (x_i, y_j) is assumed to satisfy a linear regression equation given by

$$\rho_{ij} = \beta_o + \beta_1 x_i + \beta_2 y_j$$

Here we make the standard assumptions, namely that

$$r_{ij} \equiv \rho_{ij} + z_{ij}$$

where the z's are independent normally distributed variates with zero mean and common variance σ^2.

Three points should be borne in mind with regard to the regression function ρ. First, and most important, is the fact that we do not have to assume that the parameters β_o, β_1, β_2 are invariant from place to place on the map, or from storm to storm at a fixed location. Whatever the parameters might be for the

storm and test plot in question, we shall estimate them from the observational data taken from that one time and place, without dependence upon climatological records. Second, the statistical test of significance will be parameter-free; that is, it will be a determinate function of observable quantities. Third, even if the distribution of the logarithm of rainfall is not very nearly normal, it is still quite reasonable to assume a normal distribution for the residual variate z, because the removal of a definite geometric pattern ρ from r could well leave normally distributed random deviations.

The hypothetical regression function ρ will be approximated by a similar function r^* where

$$r^* = b_o + b_1x + b_2y$$

and the constants b_o, b_1, b_2 will be determined from the data. Seeding will be confined to two points, (0,0) and (0,1). The remaining seven points will be used to fit the constants. Since these points will not be seeded, the function r^* will provide an estimate of the amount of rainfall which would occur naturally. Briefly, the object of the design is this: The experimental control will be furnished by the regression function, and the measure of effectiveness will be based upon the deviations of actual rainfall from the regression function at the two seeded points. The experiment will be replicated as often as necessary to build up a conclusive body of evidence. The statistical theory and computational procedure are presented in the following paragraphs.

From established principles of mathematical expectation, it can be proved that an unbiased estimate of the residual variance σ^2 is given by s^2, where

$$s^2 = \frac{\Sigma(r_{ij} - r^*_{ij})^2}{N - 3} = \frac{\Sigma(r_{ij} - r^*_{ij})^2}{4}$$

Being a function of random variables, the statistic s^2 is itself a random variable. Its probability distribution is simply related to that of chi-square, inasmuch as the variate $4s^2/\sigma^2$ is distributed precisely as chi-square with four degrees of freedom.

If, under the assumptions stated, a statistically derived regression function r^* is applied to fresh data, the attendant deviation $r_{ij} - r^*_{ij}$, regarded as a random variable, will be normally distributed with zero mean, but its

variance will be a function of the sampling variances and covariances of the estimated regression constants b_o, b_1, b_2. The variance of $r_{ij} - r^*_{ij}$ is

$$E(r_{ij} - r^*_{ij})^2 = E(r_{ij} - \rho_{ij})^2 + E(r^*_{ij} - \rho_{ij})^2$$

$$= \sigma^2 + E(r^*_{ij} - \rho_{ij})^2$$

By direct substitution

$$E(r^*_{ij} - \rho_{ij})^2 = E[(b_o - \beta_o) + (b_1 - \beta_1)x_i + (b_2 - \beta_2)y_j]^2$$

To evaluate this quadratic, matrix notation is convenient. Introduce the matrix

$$A = \left\| \begin{array}{ccc} N & \Sigma x & \Sigma y \\ \Sigma x & \Sigma x^2 & \Sigma xy \\ \Sigma y & \Sigma xy & \Sigma y^2 \end{array} \right\|$$

and denote its inverse by A^{-1}. The first element N of the matrix A represents the sample size, which in this case is 7. Define the row vector

$$v = (1 \quad x_i \quad y_j)$$

and let v' stand for its transpose. Then it can be demonstrated that

$$E[(b_o - \beta_o) + (b_1 - \beta_1)x_i + (b_2 - \beta_2)y_j]^2 = \sigma^2 \, vA^{-1}v'$$

Hence the variance of $r_{ij}-r^*_{ij}$ is

$$E(r_{ij} - r^*_{ij})^2 = \sigma^2(1 + vA^{-1}v')$$

The latter quantity in parentheses is a numerical constant which can be computed without reference to the rainfall amounts.

Consider now the deviations

$$u_o \equiv r_{22} - r^*_{22}$$
$$u_1 \equiv r_{12} - r^*_{12}$$

The variance of u_o is $k_o\sigma^2$ and that of u_1 is $k_1\sigma^2$ where

$$k_o = (1 + v_o A^{-1} v'_o)$$

$$k_1 = (1 + v_1 A^{-1} v'_1)$$

in which $v_o = (1 \quad 0 \quad 0)$, $v_1 = (1 \quad 0 \quad 1)$. We have observed that u_o and u_1 are normal variates, each having a mean of zero. If we now construct the linear combination

$$\bar{u} = \frac{u_o/\sqrt{k_o} + u_1/\sqrt{k_1}}{2}$$

this too will be normally distributed with zero mean and its variance will be $\sigma^2/2$.

The two statistics s^2 and $\bar{u}$ are independently distributed. Accordingly, if we set

$$t = \frac{\bar{u}/\sqrt{\sigma^2/2}}{\sqrt{s^2/\sigma^2}} = \frac{\bar{u}\sqrt{2}}{\sqrt{s^2}}$$

we arrive at a parameter-free statistic having the well-known t-distribution with four degrees of freedom. In other words, the probability density function of t is

$$f(t) = \tfrac{3}{8}(1 + t^2/4)^{-5/2} = 12(4 + t^2)^{-5/2}$$

If seeding increases rainfall then $\bar{u}$, and consequently t, should tend to exceed zero. Therefore, the appropriate measure of significance is the probability (under the null hypothesis) that a value of t at least as great algebraically as the one obtained in the seeding experiment would occur by chance. Specifically, if in the experiment we find that $t = \tau$, then the measure of significance $p(\tau)$ is given by

$$p(\tau) = \int_{\tau}^{\infty} f(t)dt = \frac{1}{2}\left[1 - \frac{(6\tau + \tau^3)}{(4 + \tau^2)^{3/2}}\right]$$

While the foregoing equation gives the exact value of p, numerical work can be

reduced by referring to suitable tables.* The curve of Figure 3 represents the density function $f(t)$, and the shaded portion under the curve represents the probability $p(\tau)$.

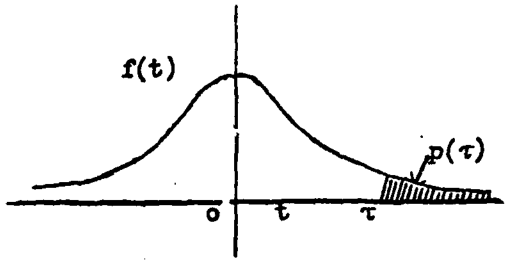

Figure 3

The experiment can be extended indefinitely by replication--that is, applying the same scheme over and over. Several nonoverlapping plots can be used in the same storm, and repeated samples can be taken from the same plot in different storms. The total evidence can then be pooled by the standard procedure of combining probabilities from independent tests. Although the technique is simple, and to a statistician intuitively clear, the non-statistician is apt to experience difficulty in appreciating its validity. Therefore we shall sketch a derivation of the method from first principles.

Suppose that two independent tests resulted in the significance measures (probabilities) p_1 and p_2 respectively. Loosely speaking, the probability of obtaining such an outcome by chance is the product p_1p_2. More precisely, however, this product is really a probability density, because p_1 and p_2 are both continuous variates. As a matter of fact, it can be shown that p_1 and p_2 are rectangularly distributed, and since they obviously range from zero to 1, the constant density value of each is unity. Construct a rectantular coordinate system, as in Figure 4, with horizontal axis p_1 and vertical axis p_2. Under the null hypothesis the points (p_1,p_2), representing the results of any pair of tests, are uniformly distributed over the unit square in the first quadrant. The rectangular hyperbola $p_1p_2 = \lambda$ defines the probability locus of all pairs of test results having equal likelihood. The combined significance measure of any pair of tests for which $p_1p_2 = \lambda$ is equal to the area of that portion of the unit square which lies below the hyperbola, for

*M. G. Kendall, The Advanced Theory of Statistics, Vol. 1; London, Charles Griffin and Co. Appendix, Table 3; p.440. The numbers in this table, for four degrees of freedom, give the appropriate significance probabilities when subtracted from unity.

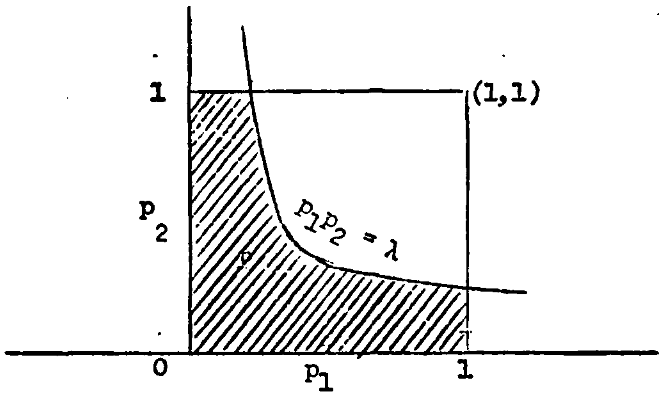

Figure 4

this area represents the total probability of obtaining some pair of values for which the likelihood is equal to or less than λ. Denoting the combined significance by P, we have

$$P = 1 - \int_{\lambda}^{1} \int_{\lambda/p_1}^{1} dp_1 dp_2 = \lambda(1 - \ln\lambda)$$

Similarly, for three independent tests, the sample space is a unit cube, within which all points have unit density. The likelihood contour is the hyperbolic surface $p_1p_2p_3 = \lambda$, and the combined significance measure is

$$P = 1 - \int_{\lambda}^{1} \int_{\lambda/p_1}^{1} \int_{\lambda/p_1p_2}^{1} dp_1 dp_2 dp_3 = \lambda\left[1 - \ln\lambda + \frac{(\ln\lambda)^2}{2}\right]$$

In general, for n tests it is easy to show that the combined significance measure is

$$P = \lambda\left[1 - \ln\lambda + \frac{(\ln\lambda)^2}{2!} - \frac{(\ln\lambda)^3}{3!} + \dots + (-1)^{n-1}\frac{(\ln\lambda)^{n-1}}{(n-1)!}\right]$$

where λ equals the product of the p's. Since very good tables of logarithms are available, P can be computed directly from the foregoing equation. However, most of the arithmetic can be avoided by using tables of the chi-square integral. For 2n degrees of freedom, the density function of chi-square is

$$g(\chi^2) = \frac{1}{2^n\Gamma(n)} (\chi^2)^{n-1} e^{-\chi^2/2}$$

and it turns out that the corresponding probability integral is

$$\int_{\chi^2}^{\infty} g(\chi^2)d\chi^2 = e^{-\chi^2/2}\left[1 + (\chi^2/2) + \frac{(\chi^2/2)^2}{2!} + \frac{(\chi^2/2)^3}{3!} + \ldots + \frac{(\chi^2/2)^{n-1}}{(n-1)!}\right]$$

Now, if we make the substitution $\lambda = e^{-\chi^2/2}$ we obtain

$$P = e^{-\chi^2/2}\left[1 + (\chi^2/2) + \frac{(\chi^2/2)^2}{2!} + \frac{(\chi^2/2)^3}{3!} + \ldots + \frac{(\chi^2/2)^{n-1}}{(n-1)!}\right] = \int_{\chi^2}^{\infty} g(\chi^2)\, d\chi^2$$

Therefore, the combined significance measure P can be evaluated from the integral of chi-square with 2n degrees of freedom if we define

$$\chi^2 = -2\ln\lambda$$

A statistician arrives at this conclusion much more expeditiously by associating with each probability p_i a chi-square equal to $-2\ln p_i$ and having two degrees of freedom. By the reproductive property of chi-square, the sum of these is another chi-square but with 2n degrees of freedom. The step of defining an individual chi-square as $-2\ln p_i$ is justified by the fact that for two degrees of freedom, chi-square is precisely equal to minus twice the logarithm of its own probability integral.

Proceeding now to specific computational details, we present a comprehensive numerical routine for the proposed grid system. The corresponding values of x_i, y_j, and r_{ij} are exhibited in Table 1, and the entries of A are

$$N = 7,\ \Sigma x = 0,\ \Sigma y = -1,\ \Sigma x^2 = 6,\ \Sigma xy = 0,\ \Sigma y^2 = 5$$

Hence

$$A = \begin{Vmatrix} 7 & 0 & -1 \\ 0 & 6 & 0 \\ -1 & 0 & 5 \end{Vmatrix}$$

and the inverse matrix is

$$A^{-1} = \frac{1}{204}\begin{Vmatrix} 30 & 0 & 6 \\ 0 & 34 & 0 \\ 6 & 0 & 42 \end{Vmatrix}$$

Tablo 1

Corresponding Values of Variates

ij	x_i	y_j	r_{ij}
11	-1	1	r_{11}
13	1	1	r_{13}
21	-1	0	r_{21}
23	1	0	r_{23}
31	-1	-1	r_{31}
32	0	-1	r_{32}
33	1	-1	r_{33}

The matrices A and A^{-1} are, of course, independent of the rainfall data. In addition, we need four functions of the observations—namely:

$$c_o = \Sigma r_{ij} = (r_{11} + r_{21} + r_{31}) + (r_{13} + r_{23} + r_{33}) + r_{32}$$

$$c_1 = \Sigma x_i r_{ij} = -(r_{11} + r_{21} + r_{31}) + (r_{13} + r_{23} + r_{33})$$

$$c_2 = \Sigma y_j r_{ij} = (r_{11} + r_{13}) - (r_{31} + r_{32} + r_{33})$$

$$\Sigma r_{ij}^2 = r_{11}^2 + r_{21}^2 + r_{31}^2 + r_{13}^2 + r_{23}^2 + r_{33}^2 + r_{32}^2$$

Formally, the estimated regression constants are given by

$$\begin{bmatrix} b_o \\ b_1 \\ b_2 \end{bmatrix} = A^{-1} \begin{bmatrix} c_o \\ c_1 \\ c_2 \end{bmatrix} .$$

In this case the solution is

$$b_o = \frac{5c_o + c_2}{34}$$

$$b_1 = \frac{c_1}{6}$$

$$b_2 = \frac{c_o + 7c_2}{34}$$

The residual sum of squares is

$$\Sigma(r_{ij} - r^*_{ij})^2 = \Sigma r_{ij}^2 - (b_o c_o + b_1 c_1 + b_2 c_2)$$

and so

$$s^2 = \frac{\Sigma r_{ij}^2 - (b_o c_o + b_1 c_1 + b_2 c_2)}{4}$$

At any point (x_i, y_j) the regression function is

$$r^*_{ij} = b_o + b_1 x_i + b_2 y_j$$

Therefore, the prediction r^*_{22} for the point (0,0) reduces to

$$r^*_{22} = b_o$$

and that for the point (0,1) becomes

$$r^*_{12} = b_o + b_2$$

Accordingly

$$u_o = r_{22} - b_o$$

$$u_1 = r_{12} - b_o - b_2$$

Given $v_o = (1 \quad 0 \quad 0)$, $v_1 = (1 \quad 0 \quad 1)$, the quantities $vA^{-1}v'$ are

$$v_o A^{-1} v'_o = \frac{5}{34}; \qquad v_1 A^{-1} v'_1 = \frac{7}{17}$$

Whereupon the constants k_o and k_1 are

$$k_o = 1 + \frac{5}{34} = \frac{39}{34}; \qquad k_1 = 1 + \frac{7}{17} = \frac{24}{17}$$

The remaining quantities are

$$\bar{u} = \frac{u_o/\sqrt{k_o} + u_1/\sqrt{k_1}}{2} = \frac{u_o\sqrt{34/39} + u_1\sqrt{17/24}}{2}$$

and

$$t = \frac{\bar{u}\sqrt{2}}{\sqrt{s^2}}$$

If no interest attaches to $\bar{u}$ and s^2 in themselves, the calculation of t can be shortened by combining terms in a different way obtaining

$$t = \frac{w_o u_o + w_1 u_1}{\sqrt{\Sigma(r_{ij} - r^*_{ij})^2}}$$

where

$$w_o = \sqrt{2/k_o} = \sqrt{68/39} = 1.32$$

$$w_1 = \sqrt{2/k_1} = \sqrt{34/24} = 1.19$$

and, of course, $\Sigma(r_{ij} - r^*_{ij})^2 = \Sigma r_{ij}{}^2 - (b_o c_o + b_1 c_1 + b_2 c_2)$. To six decimals the weights w_o, w_1 are 1.320451, 1.190238 respectively. Thus the figures given above are actually good to three decimals, which should suffice forpractical purposes.

Operational considerations might dictate the use of a fixed network of points for all experiments. In that case the test plots could be chosen as small compact areas containing nine points, with the two test points oriented as before. An important condition to be satisfied is that none of the nonseeded points must be downwind from the test points. Because the network is no longer assumed to be strictly regular, the double subscript notation is inappropriate. Instead, we shall number the points 0, 1, 2, ..., 8, the first two being the test points. This scheme is represented in Table 2.

Table 2

Fixed Network Scheme

Serial Number	x	y	r	
0	x_o	y_o	r_o	Seeded
1	x_1	y_1	r_1	
2	x_2	y_2	r_2	Nonseeded
3	x_3	y_3	r_3	
4	x_4	y_4	r_4	
5	x_5	y_5	r_5	
6	x_6	y_6	r_6	
7	x_7	y_7	r_7	
8	x_8	y_8	r_8	

Although the regression constants b_o, b_1, b_2 and the inverse matrix A^{-1} are formally involved in the determination of s^2, $\bar{u}$, and t, they need not be derived explicitly. An efficient computational procedure is as follows:

Compute the sums of first powers, squares, and cross products--

$$\sum_2^8 x_i,\ \sum_2^8 y_i,\ \sum_2^8 r_i,\ \sum_2^8 x_i^2,\ \sum_2^8 y_i^2,\ \sum_2^8 r_i^2,\ \sum_2^8 x_i y_i,\ \sum_2^8 x_i r_i,\ \sum_2^8 y_i r_i$$

Then set up the initial matrix as shown below, it being understood that all summations go from 2 to 8.

$$\text{Initial Matrix} = \begin{Vmatrix} 7 & \Sigma x & \Sigma y & \Sigma r & 1 & 1 \\ \Sigma x & \Sigma x^2 & \Sigma xy & \Sigma xr & x_o & x_1 \\ \Sigma y & \Sigma xy & \Sigma y^2 & \Sigma yr & y_o & y_1 \\ \Sigma r & \Sigma xr & \Sigma yr & — & — & — \\ 1 & x_o & y_o & — & — & — \\ 1 & x_1 & y_1 & — & — & — \end{Vmatrix}$$

In the lower right hand corner of the initial matrix, the dashes represent blanks, which are to be ignored in the computations. Now derive the Crout auxiliary matrix according to the directions given in Marchant Methods, MM 182. Representing the general element of this auxiliary matrix by the symbol e_{ij} we have

$$\text{Auxiliary Matrix} = \begin{Vmatrix} e_{11} & e_{12} & e_{13} & e_{14} & e_{15} & e_{16} \\ e_{21} & e_{22} & e_{23} & e_{24} & e_{25} & e_{26} \\ e_{31} & e_{32} & e_{33} & e_{34} & e_{35} & e_{36} \\ e_{41} & e_{42} & e_{43} & — & — & — \\ e_{51} & e_{52} & e_{53} & — & — & — \\ e_{61} & e_{62} & e_{63} & — & — & — \end{Vmatrix}$$

Then

$$4s^2 = \sum_2^8 (r_i - r^*_i)^2 = \sum_2^8 r_i^2 - (e_{41}e_{14} + e_{42}e_{24} + e_{43}e_{34})$$

$$u_o = r_o - r^*_o = r_o - (e_{51}e_{14} + e_{52}e_{24} + e_{53}e_{34})$$

$$u_1 = r_1 - r^*_1 = r_1 - (e_{61}e_{14} + e_{62}e_{24} + e_{63}e_{34})$$

$$k_o = 1 + e_{51}e_{15} + e_{52}e_{25} + e_{53}e_{35}$$

$$k_1 = 1 + e_{61}e_{16} + e_{62}e_{26} + e_{63}e_{36}$$

The rest of the calculations are performed as previously. The mathematical justification of this computational short-cut requires too much matrix theory to be gone into here, but a numerical verification is furnished by the following example:

A square plot, 30 miles on a side, was chosen in central Iowa. As previously discussed, this was subdivided into four grid squares 15 miles on a side. From a contour map of hourly precipitation as of 4 a.m. on 18 June 1950 the rainfall amounts were read at the intersection points of the grid. The actual values are shown in Figure 5a, and the corresponding natural logarithms, each increased by 10, are indicated in Figure 5b. It is legitimate to add 10, or any other arbitrary constant, to the logarithms, because the regression function adjusts to any linear change of variable; one should be careful, however, not to forget to add 10 to the logarithm when the rainfall amount exceeds unity. We shall perform the computations first by the method given for the square grid and then by the general method using the Crout auxiliary matrix. In order to insure numerical agreement between the two systems, we shall carry more figures than would otherwise be needed.

$R_{11} = .19$	$R_{12} = .18$	$R_{13} = .18$
$R_{21} = .27$	$R_{22} = .23$	$R_{23} = .22$
$R_{31} = .32$	$R_{32} = .30$	$R_{33} = .27$

Figure 5a

8.339	8.285	8.285
8.691	8.530	8.486
8.861	8.796	8.691

Figure 5b

Since all nine of the observations represent natural precipitation, we should expect the value of t to support the null hypothesis that ordinary circumstances are in force.

The functions c_o, c_1, etc. are

$$c_o = 60.149$$

$$c_1 = -.429$$

$$c_2 = -9.724$$

$$\Sigma r^2 = 517.146241$$

The regression constants are

$$b_o = \frac{5c_o + c_2}{34} = \frac{291.021}{34} = 8.559441$$

$$b_1 = \frac{c_1}{6} = \frac{-.429}{6} = -.071500$$

$$b_2 = \frac{c_o + 7c_2}{34} = \frac{-7.919}{34} = -.232912$$

The sum of squares of residuals is

$$\Sigma(r-r^*)^2 = \Sigma r^2 - (b_oc_o + b_1c_1 + b_2c_2)$$

$$= 517.146241 - 517.137326$$

$$= .008915$$

and

$$s^2 = \frac{\Sigma(r-r^*)^2}{4} = .002229$$

The deviations u_o and u_1 and the linear combination $\bar{u}$ are

$$u_o = r_{22} - b_o = 8.530 - 8.559441 = -.029441$$

$$u_1 = r_{12} - b_o - b_2 = 8.285 - 8.326529 = -.041529$$

$$\bar{u} = \frac{u_o\sqrt{34/39} + u_1\sqrt{17/24}}{2} = \frac{-.062441}{2} = -.031220$$

In connection with $\bar{u}$, we note for future reference that

$$k_o = 39/34 = 1.147059, \qquad k_1 = 24/17 = 1.411765$$

Now we compute the value of t by the two formulas given above, with and without explicit use of $\bar{u}$ and s^2. By the first formula,

$$t = \frac{\bar{u}\sqrt{2}}{\sqrt{s^2}} = \frac{-.031220\sqrt{2}}{.047212} = \frac{-.044152}{.047212} = -.9352$$

By the second formula, using w_o, w_1 to six decimals ($w_o = 1.320451$, $w_1 = 1.190238$) we have

$$\sqrt{\Sigma(r - r^*)^2} = \sqrt{.008915} = .094419$$

$$t = \frac{w_ou_o + w_1u_1}{\sqrt{\Sigma(r - r^*)^2}} = \frac{-.088305}{.094419} = -.9352$$

Again, by the second formula but using w_o, w_1 to two decimals

$$t = \frac{.08828}{.09442} = -.9350$$

Thus the two-decimal weights are sufficiently accurate.

The general method is designed to take care of irregularly spaced points but, of course, it will give the same answer as the first method where the latter is applicable. The arrangement of data for the general method is shown in Table 3.

Table 3

Basic Data for General Method

Serial Number	x	y	r	
0	0	0	8.530	Test Points
1	0	1	8.285	
2	0	-1	8.796	
3	-1	1	8.339	
4	-1	0	8.691	
5	-1	-1	8.861	
6	1	1	8.285	
7	1	0	8.486	
8	1	-1	8.691	

The initial matrix is

$$\text{Initial Matrix} = \begin{Vmatrix} 7 & 0 & -1 & 60.149 & 1 & 1 \\ 0 & 6 & 0 & -.429 & 0 & 0 \\ -1 & 0 & 5 & -9.724 & 0 & 1 \\ 60.149 & -.429 & -9.724 & \text{—} & - & - \\ 1 & 0 & 0 & \text{—} & - & - \\ 1 & 0 & 1 & \text{—} & - & - \end{Vmatrix}$$

The corresponding Crout auxiliary matrix is